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AUTOBIOGRAPHICAL REMINISCENCES OF ROBERT ROSEN

I have never enjoyed writing as an activity in itself, though over the course of time I have done a considerable amount of it. Already as a graduate student, I begrudged the time and effort it required; begrudged it because I already knew what I was merely now repeating and expositing, and because I felt the effort expended on mere repetition could more profitably be invested in trying to find out something new. I still feel that way.

I was early persuaded to act otherwise by my mentor Nicolas Rashevsky, then my Major Professor at the University of Chicago. He did not tell me that I was being "impractical" in such an attitude; that my scientific career and status would depend on a burgeoning publication list. He must have known that such arguments would cut little ice with me. He did not merely demand it, as he was in a position to do. Rather, he invoked the Categorical Imperative; he pointed out that if others had acted as I was proposing to act, then I could have no access to their accumulated knowledge and wisdom, and therefore could not learn from them. I had no answer to this, so I conceded, even while admiring his artistry in choosing that one particular argument to which I would have to acquiesce.

So I thereby acknowledged a duty to report. That is how I view my scientific writing – as reporting. It is not proselytizing; it is not advocacy; it is not even instruction. And it is in that light that I have prepared the present article, even though it is about me, and not so much about what I know. I hope the reader will appreciate this spirit from the outset.

Though the reporting of my scientific work, including the material to follow, is simply the discharge of a Kantian duty, I feel quite otherwise about the work itself. I have never regarded my attachment to science as constituting in any conventional sense a "career" or vocation or job. To me, it is an Imperative in itself, more akin to what theologians refer to as a "calling", something which would be corrupted and defiled by being subordinated to any such personal considerations as constitute professional aggrandizement. Indeed, it has always seemed to me a kind of miracle that people were willing to pay me to do what I wanted to do, and would have done, anyway. On the rare occasions when, at the urging of others, I have violated this Imperative for parochial "career" considerations, I have invariably come to grief. Whatever scientific powers I possess cannot be employed to such personal ends; like witchcraft, they can only be directed outward, and cannot be invoked on one's own behalf. Therein lies their strength, and also, in another sense, their curse.

I must spend some time in explaining this Imperative, since it constitutes, as it were, the invisible steel skeleton which has guided and which supports the otherwise perhaps inexplicable diversity of my individual scientific activities. To me, on the other hand, these activities comprise a self-evident unity, each one forced on me by the preceding ones, and by that underlying skeleton, of which I am never unaware.

Einstein has reported how his scientific instincts were galvanized in early childhood by a compass needle. What the compass needle did for Einstein was accomplished for me by humble living things: beetles and crickets and caterpillars. Among my earliest memories are walks through wild and overgrown vacant lots which dotted the asphalt Brooklyn landscape into which I was born. Under ever rock was a new and thrilling universe of living things. From these experiences was born an eternal passion, a lust, to understand why these things, in their separate ways, were alive, while the rock was not. The rocks were themselves mildly interesting, but in a bland, impersonal way; it was the life which was the compelling challenge to me. If I could find out what the life was, I would know what the rocks were, but, as it even then seemed to me, not the other way around.

When I was five or six, I was taken to see the Disney film "Fantasia". I remember being mesmerized by the panoply of life through the eons, which the Disney cartoonists set to Stravinsky's "Rite of Spring". This was worth spending a lifetime with. Though I did not even know the word at the time, had already determined to become a Biologist.

By that age, I had long since learned not to ask complicated questions of the adults around me, either family or teachers, because they didn't know. Although I had no idea then where they came from, books seemed more authoritative, so I began reading anything I could find dealing with life and the living. Unconsciously, I was casting about for information, not only about this life which fascinated me, but on how one best went about understanding it; information on how to be the kind of Biologist I increasingly aspired to be.

As I read, and assimilated, and integrated, my perspective continually shifted. At first, I thought I would be what was then called a "naturalist"; continuing to study and observe organisms in themselves. But lots of other people had been doing that for a long time, and they did not know (or even care) what the life was; perhaps the answer was not there, however much fun such studies might be. Then I thought I would be a paleontologist, going back and back to the historical beginnings. That phase lasted somewhat longer, until I realized the answers might not lie in historical records either. By then I was reading about genetics and biochemistry, about metabolism and physiology and embryology and the intriguing possibility disclosed itself that, in the inner workings of what was alive would reside the best way to get at what made it alive.

Thus, I entered into a prolonged empirical phase, essentially a reductionist phase, dominated by biochemistry. Although it may surprise some people, I acquired a fairly extensive laboratory capability during those years. By this time I was in high school, a "Biology major" at Stuyvesant, taking elective courses in analytical and organic chemistry, and using the laboratory facilities for my own purposes when they were unoccupied. I became rather notorious for these activities, but became good enough to be utilized informally as a laboratory assistant at faculty demonstrations of techniques.

It was the attempt to understand what I was doing in these empirical activities (basically, to understand what a molecule was) that led me to instruct myself in physical chemistry, and then to the physics which underlay it, and then, fatefully, into the mathematics in which the physics was expressed. I somehow came quickly to the conclusion that, wherever the life was, the avenue for finding it was somewhere in there. That abruptly ended my empirical phase, and I decided that henceforth, I must become proficient enough in that mathematical language to understand, to the root, what realities were being, or indeed could be, expressed through it.

Up to that point, I had had only the most perfunctory interest in the sciences of the inanimate; these were the rocks again, and not the life. Suddenly, it now seemed a matter of urgent necessity to master these things. To facilitate acquiring such a mastery, it seemed the most natural thing in the world to change my major. So I blithely shifted out of biology and into mathematics. It felt perfectly right to do so, and I regarded it as the merest tactical device in the service of the unchanging strategy I was groping for.

I couldn't explain to anyone that I was not "abandoning" biology for mathematics. I well remember vainly trying to explain it to the Guidance Counselor, who regarded us as high-strung, unstable adolescents, and to whom any change in behavior patterns was an ominous portent of disaster. Somehow, I managed to convince her that there was nothing sinister in what I was doing, but henceforth I had the feeling of being watched closely. I did not like it.

Thus began a long period of total immersion, in both pure mathematics and in mathematical physics, which lasted almost unbroken until the end of my student days. I was accepted by these constituencies as one of them, but at the cost of not disclosing my ulterior motivations for being there. I felt much like the Englishman who visited Mecca during the Hadj: disguised as an Arab, and knowing he would be torn to pieces if his true identity were disclosed. Indeed, except for the required year of college biology, I took no more formal courses in the subject until almost done with graduate school, and then only to satisfy formal degree requirements. But to me, this posed no hardship; biology was "my" subject, which I could pick up again at any time, whenever my extended tactical detour was through. In any case, there was nothing in any of those college biology courses which I didn't already know, many times over.

I quickly came to recognize that my instincts had been correct: that the mathematical universe had much of value to offer me, which could not be acquired in any other way. I saw that mathematical thought, though nominally garbed in syllogistic dress, was really about patterns; you had to learn to see the patterns through the garb. That was what they called "mathematical maturity". I learned that it was from such patterns that the insights and theorems really sprang, and I learned to focus on the former rather than the latter. More of this in a moment.

After a few years of such acclimation I came to focus my interest on the theory of operators and of operator algebras. This was beautiful in itself, but it was also the language of quantum mechanics, then the last and most exciting word in theoretical physics. The science of the rocks, and hence, it was impressively argued, of everything. I resolved to do my graduate work at the University of Chicago, because its Department of Mathematics was then the strongest in the country in this field; get my PhD there; and then would turn back to my Imperative, apparently in the form of getting the life to emerge from the rocks.

As it happened, I did not go to Chicago immediately after graduating from Brooklyn College, for familial reasons. While growing up, I had come to love New York and its infinite diversities, and was fond of boasting that there was nothing which could not be found in that city, if one only knew where and how to look. My parents, who were somehow terrified of my "abandoning" New York for Chicago, threw these words in my teeth: why go to Chicago when everything was already in New York? We came to a compromise: I would spend my first graduate year in New York, if I found it unsatisfactory, I could leave for Chicago unopposed.

I investigated several possibilities. One was the Courant Institute. They were horrified by even the suggestion of biology, and offered me instead a PhD program in fluid dynamics, which in their view, exhausted the universe. I settled rather on Columbia University, which at least on paper had a bit of a program in operator theory. In some ways, it was fortunate for me that I did so, as I will explain below. But in general, the year was one of intense academic frustration: I was learning little there, and I came to hate the sterile ambience of the place.

There was on the Columbia faculty one person who was described as a "biophysicist." I went to see him, hoping to get first-hand advice, about what biophysicists did. In fact, this person was a muscle physiologist, who had a little laboratory in the attic on the 14th floor of the physics building, Pupin Hall. It was a true attic: dark, damp, and disorderly. I found the little cubbyhole which housed this person, knowing already I was on a fruitless errand. But I went through with it, trying to explain my intentions, however imperfectly. To this day, I remember his contemptuous retort: "We don't do any of that theoretical stuff around here; we keep our feet on the ground." It was all I could do to keep from laughing in his face, at the sudden vision of ourselves, 14 stories up in a corner of an attic, 'keeping our feet on the ground'. It's a picture which often comes to mind when dealing with experimentalists, even now. In any case, I stuck out the remainder of the academic year, picked up a perfunctory MA, and left for a new life in Chicago.

After the sterility of Columbia, and indeed of the 4 years of college which had preceded it, the University of Chicago was like an explosion of light. The sheer intellectual ferment of the place was like nothing else in my experience, filled with the excitement of new things to learn. But things were to turn even better for me, from a completely unforeseen quarter. I had long known of the existence of a Committee on Mathematical Biology at the University of Chicago. I knew Rashevsky's book, "Mathematical Biophysics". And I knew that this was very far from the sorts of things I had in mind. In my view, all these activities were focused entirely on epiphenomena of life, and not on the life itself. Blood flow in arteries? Propagation of action potentials? This is not the stuff of life; this was back to the rocks again. Indeed, such concerns seemed diametrically opposite to my own; knowing about them only strengthened my resolve to persist in my own strategy and begin afresh.

Nevertheless, soon after I arrived in Chicago, and almost by accident, I obtained an appointment to see Rashevsky himself. I expected it would be like my encounter with the Columbia "biophysicist". But it was not. Rashevsky offered me something I had never received or solicited or expected from any external quarter: encouragement. He informed me that his own views had changed radically over the past few years, and had led him to a new approach which he had christened Relational Biology. He gave me his keystone paper, then only 2-years-old and entitled, provocatively, "Topology and Life", to read. This turned out to be the only thing I had ever come across that was in my ballpark; consonant with my own Imperative. After a few more discussions, Rashevsky offered me a small fellowship, an office of my own, and absolute carte blanche in preparation of a dissertation, in return for taking my PhD in his Committee.

After a day or two of reconsideration of my alternatives, I accepted this offer. My feeling was that I had already accomplished my purpose in studying Mathematics; I regarded myself as fully independent and fluent in that language. To persist in Mathematics, in the face of Rashevsky's offer, would gain me little and would in fact slow me down. So I transferred out of the Department of Mathematics, and into the Committee on Mathematical Biology. Once again, everyone thought I must have lost my mind, and once again, I could not explain. But by my lights, it was the only correct thing to do. I received my PhD in Mathematical Biology 2 years later.

Upon entering the Committee, in the fall of 1957, I at last felt fully free to unleash myself in the pursuit of my Imperative, too inexperienced to be daunted by what I was proposing to undertake. I felt, what I still feel, that I had at least an even chance of success; that my inherent intellectual equipment and the scientific capabilities I had accumulated gave me perspectives which no one else had, and that if I failed, it would only be my own fault. In short, I was already the Biologist I had aspired to become, and it was now time to put those arts to the test.

The next 2 years consisted of an absolute orgy, a frenzy, of activity. I simultaneously embarked along at least a half-dozen fronts. Much of this work was only published years later, if at all. Early in 1957, I had discovered the (M,R)-systems, and developed some of their extraordinary properties; this work, published in 1958 and 1959, became my dissertation. I began to explore the quantum-mechanical dictum that material events consisted of observables being evaluated on states, as the tangible bridge between the rocks and the life. I became aware of the strange epistemological status of Church's Thesis, and began to explore its actual implications. I did some (abortive) work on algebraic aspects of biological coding schemes, and decided on morphogenesis, a uniquely biological phenomenon, as my testing ground for general theoretical ideas in biology. Most of my subsequent scientific work has been based, in large part, on the foundations I established in those 2 years.

I felt then, and continue to feel, that none of this work was in any way "speculative". Indeed, I believe that theory is the antithesis of "speculation", despite the confusion between the two in the minds of those who do speculate. Nor have I ever believed that theory and "practice" were in any way adversarial. What I do believe is that "practice," in the form of observation and experiment, cannot constitute or replace theory, and that most of the basic questions of science, especially in Biology, fall quite outside the ken of "practice", in the usual sense. My own life would have been made much simpler if empirics alone would suffice for my Imperative.

It might be well to spend a moment on the general scientific ambience of those years, since they were exciting in a way which can barely be dreamed of today. On the "theoretical" side there was Schrödinger's little book, "What is Life?", in which, however

Schrödinger did little but repeat the words and outlook of his student Max Delbruck. From my viewpoint, Schrödinger did not begin to answer his question; he rather equated "life" with a kind of stability, and asserted that "life" must be molecular because molecules are stable too. In the late 1940's appeared Norbert Weiner's book "Cybernetics", invoking a new technological language the Cartesian equation between animal and machine. There was the confluence then crystallizing between foundational work in mathematics itself (exemplified primarily in the Turing machine and the execution of algorithms) and digital computation, and the brain, embodied in the neural networks proposed decades earlier by Rashevsky himself; all this roughly constituted the province of "Automata Theory". There was the Theory of Information of Shannon. There was Game Theory. And in Biology itself, there was the increasing inroad of digital thought, of hardware and software, which were the concomitants of "molecular biology". And of course, part and parcel of all this, was the newly emerging strain of General Systems Theory, associated especially with names like Bertalanffy and Ashby. A yeasty mix indeed.

To me, though, and in the light of my own Imperative, all these things were potential colors for my palette, but not the palette itself. I regarded them as monochromes, individually perhaps lovely in themselves, but not to be applied when a different hue was required. I could not share the prevailing sentiment that these developments, either individually or collectively, would paint themselves into the picture I was striving after. Rather, I felt it was the picture which would illuminate them.

Indeed, my own scientific work of those years was pushing me against these currents. Consider, for example, the discovery which most shocked me in those days, when I still had unlimited faith in the physicists' Quantum Mechanics as the ultimate bridge between the rocks and the life. I had long been puzzled by the fact that the state spaces they posited for every material system were mathematically indistinguishable, abstractly identical, isomorphic (they are all separable Hilbert spaces, and there is objectively only one such). The perceptible differences between material systems must thus lie only in a "choice of co-ordinates", and in how the observables, the Hermitian operators on states, were labelled; hence in what, mathematically, constituted the subjective. This, in turn, implied that we could get from one system to any other by relabelling these observables; by calling one of them, say, a Hamiltonian instead of another. Hence, any system would appear to be any other system, if only we looked at them with the "right eyes". The only escape from this disturbing conclusion seemed to be to limit the universality of Quantum Mechanics itself... or what is the same thing, enlarge what can constitute an observable, or an observation, or a state.

I was unprepared to do this for a long time. But I was forced to it by the following considerations, which I discovered in 1959. As I have already noted, whatever else Quantum Mechanics say, it asserts that "information" about any material phenomenon consists of observables evaluated on states. Hence, *a fortiori*, "genetic information" must be of that character too, and this must provide the material, physical basis of the formal "coding schemes" which then so preoccupied everyone. So I tried to find what the observables had to be in order to manifest this "information". The shock was in discovering that the families of observables I characterized in that way could not contain anything which behaved like a Hamiltonian. And, of course, without a Hamiltonian, you cannot even get started in doing traditional Quantum Mechanics. In a sense, what I then showed was that Quantum Theory and Quantum Mechanics do not coincide, and that the former was much bigger than the latter.

At the root of these considerations is the indissoluble dependence of Quantum Mechanics upon energy conservation; that is what a Hamiltonian expresses. What happens in rocks seems to fall within such structures; what happens in life, as I showed then, and more sharply later, need not. There was an immediate parallel with the "open systems" of Bertalanffy and their devastating challenge to the Second Law of Thermodynamics; it was not that the Law was wrong – it simply did not apply. I would say that, today, there is still no satisfactory "physics" of open systems, primarily because people persist in thinking of closed systems as fundamental, and of open ones as simply closed ones canonically perturbed.

At any rate, such considerations provided the soil for a constant preoccupation with when, and under what circumstances, two systems could be considered in any sense identical. Such studies ran a gamut from the physics of the Gibbs Paradox, and the objectivity of entropy, to considerations of similitude and conjugacy.

Such considerations, and many others like them, from many different perspectives, led me away from the facile Reductionisms which almost all of my colleagues were rushing to embrace, and which they identified with science itself. From my perspectives, physics could not swallow Biology; rather, any attempt to do so would have to radically transform physics.

Fortunately, I had a positive alternative to such negative, pessimistic conclusions, in the spirit of Rashevsky's Relational Biology, and manifested in my own (M,R)-systems. As I have characterized this spirit, it involves "throwing away the physics and keeping the organization," instead of the reverse. What remains then is an abstract pattern of functional organization, which has properties of its own, independent of any particular way it might be materially *realized*. Indeed, it is what remains invariant in the class of all such material realizations, and hence characterizes that class. It is my ultimate object of study; it, and not those material objects which happened to be available to realize it.

To me, such patterns, and the elements and relations which comprise them, are as real and objective and perceptible as the products of any Reductionistic fragmentation; indeed, in some ways more so. In my view, a science too narrowly construed to encompass them from the outset is too narrow to do Biology in, just as narrow identification of mathematics with computability excludes thereby almost all of mathematics. More of this later.

The study of these (M,R)-systems brought my mathematical training and instincts to uses I could not have foreseen. For one thing, the diagrams which expressed them were in themselves an immediate invocation of the Theory of Categories. I had started to imbibe this theory during my otherwise wasted year at Columbia University. The graduate algebra course I took during that year was taught by Samuel Eilenberg, and was really a course in Category Theory: sets, operations, and structure-preserving transformations. Eilenberg, of course, was one of the creators of Category Theory. The other creator, Saunders MacLane, was at Chicago, where I imbibed much more. I became intrigued by the historical roots of the Theory, which had grown out of the attempt to make algebraic "models" of geometric objects in order to discriminate between them. It expressed in a purely mathematical realm the patterns of relations, between objects and models, and between one model and another, which I was trying to find in the realm of the living. The numbers (e.g., Betti numbers) which came out of Algebraic Topology were like the observables of material nature, but there was much more underneath them. It has been one of my primary ongoing concerns to make all this clear.

My adaptation of Category Theory to the (M,R)-systems, and indeed my utilization of Category Theory itself as a kind of framework for the notion of modelling in general, is typical of how I have used my mathematical tools over the years. Not so much in the making of particular kinds of models of particular biological phenomena (although I have done a substantial amount of that), or the invocation of specific theorems from specific mathematical domains (although I have done that too) but rather an invocation of the entailment patterns from which the theorems arise, or sometimes do not arise. So I seldom have occasion to invoke a particular theorem from Algebraic Topology (say); what is more germane to me is the relation established between a space and its models, and between one model and another, and why such relations hold.

Indeed, I have come to regard models in general as a natural but profound extension of the concept of observability, as the physicist understands it. A model indeed represents to me an inherent adjective, or property, or quality, or attribute, of the system being modelled; what the old philosophers called an essence, no less than any measured value of some magnitude does. But rather than trying to reduce every model to such measured values, or alternatively, trying to syntactically build every model out of such numerical observables, I have had to proceed in quite a different way. Indeed, it has turned out that most qualities of interest to me, were simply not expressible in such limited terms. One must follow one's "observables" to assume values other than mere numbers; to assume values in inferential patterns (in models, in short), and at the same time allow the referents of such observables to be other than conventional reductionistic fragments. Once again, none of this seems to me in any way "speculative"; it is as firmly grounded in observation as any reductionistic scheme. But it involves a notion of "observation" far more broadly conceived than has been usual, and tailored to the demands of Biology; traditional concepts of observability, and the kinds of models which can be based on them, appear in this light as very, very "special" indeed.

Thus, I have come to partition Biology into that which depends on an underlying relational pattern (e.g. an (M,R)-system independent of how it is realized, and that which depends upon the material details of a particular realization (and of course, that which depends on both). And of course, the word "realization" admits a great deal of latitude. For instance, I have come to believe that social structures, as things in themselves, realize many of the relational patterns which individual organisms have. To that extent, we can learn deep things about each by treating the one as a surrogate for the other, however different they may appear in exclusively material terms. It was in exactly that spirit that I undertook, for example, a long-term study of "anticipatory systems", which is still going on.

My concerns with "anticipation", in which what is happening now seems determined by something about the future, are worth describing in more detail. Anticipatory behavior is in fact damned as "acausal", because causality is construed precisely as allowing only the past to affect the present. I initially softened this by interposing a "predictive model" as a transducer between now and later. But nevertheless, the presumed telic or finalistic aspects of anticipation seemed to violate the one-way causal flow on which "objective science" itself is presumed to rest. And I noticed that my own (M,R)-systems have an inherent anticipatory aspect, built into their very organization.

Once again, my mathematical experience served to illuminate this situation, mathematics, the quintessence of what is objective. In mathematics, the analog of anticipation is *impredicativity*: a situation in which what is defined depends essentially on having it available from the outset. The associated "self-references", in which something is getting outside a single, one-way, coherent time-frame, can lead (and have led) to devastating paradoxes. Russel called them "vicious circles", and it was believed that the salvation of mathematics itself depended on eliminating them; somehow straightening out the impredicative loops, and proceeding in a purely syntactic way only from "past" through "present" to "future". Indeed, it was part of the allure of the algorithm, embodied in the machine, that it could only manifest this kind of flow, from input to output, and impredicativities, by their very nature, could not arise in them.

The trouble with this is that by thus "saving" mathematics from impredicativity by indentifying it with what machines can do (i.e. with pure syntax, or symbol manipulation, or word processing) the cost is relinquishing most of mathematics itself. In a certain suggestive language, there are more things in the "mathematical universe" than can be projected down predicatively into a single coherent time-frame. This is a very Platonic thing to say, but it is still true. And I believe Biology shows that it is likewise true in the causal realms of material reality as well.

My (M,R)-systems inherently manifest such an impredicative loop; one which cannot be straightened out without losing everything. They are thus not approachable via "machines" in the usual sense; they are not purely syntactic objects. They are what I call *complex* – they must have non-computable models. I would argue that, precisely by excluding temporally closed causal loops, and indeed by indentifying this exclusion with science itself, we have lost not only life, in my sense, but most of its material basis, its physics, as well. To invoke a parallel mentioned earlier: just as the "closed system" is too impoverished, too special, to be a basis for (say) the physics of morphogenesis, exactly so is the simple system, one which can be described entirely as software to a machine, too impoverished to accommodate the living. In fact, these two situations are closely related, but it would take too long to explain that relation here.

Now, let me turn to some other matters which merit reporting. As Rashevsky pointed out to me all those years ago, I'm not in the game alone. If I have made myself the scientist, the biologist, I originally aspired to be, I cannot take the entire credit, though I must entirely assume any blame. I have received much assistance and support from the communities to which I have necessarily belonged, including that of some very great minds. This in turn leads me to talk a bit about the scientific community itself, such as it is, and about the institutions which are supposed to house and support them.

I have generally regarded the University as my natural habitat, and my interests and capabilites of sufficient breadth so that I could fit in smoothly, and to mutual advantage, almost anywhere. All this seems to be becoming less and less true as time goes on. Nevertheless, I have had the benefit of participating in at least three extraordinary communities, organized around extraordinary personalities. The first of them, of course, was the Committee on Mathematical Biology at the University of Chicago, created and maintained by Rashevsky. All told, I spent about a decade in this community, first as a student, then as a Research Associate, then as Assistant Professor. For me, the Committee stopped existing when Rashevsky was driven out of it. For a long time now, it has not existed in any form at all.

I was fortunate to find for a while another congenial habitat, in the complex of activities which nucleated around the Center for Theoretical Biology at the State University of New York at

Buffalo. The personality here was that of James F. Danielli. That lasted another decade, until Danielli was driven out, and the Center abolished. At Buffalo, I also had the opportunity to create and administer a graduate program in what was called "Biomathematics," although I have always disliked that word (much as I have also disliked being nicknamed "Bob", incidentally, which nearly everyone who has ever known me uses, and which my parents and older relatives lengthened to an even more ignominious version, "Bobby"... In my opinion, "Robert" has much more poetry to it. However, I suppose it is, alas, rather too late to make such stipulations.). I look back with some pride in this program, since it was the best of its kind in North America; best because it was the most cohesive, the most comprehensive, and at the same time the most individualized. So I will take a moment to describe it.

The program was open to anyone, in any of the dozen or so participating Departments, who wanted to work in the area; anything from population dynamics to biochemical control. There was a core curriculum, which everyone was expected to take, and which consisted of those concepts I felt basic to any specialization. That curriculum consisted of five courses, of which I taught four myself, organized around the concept of *stability*. The basic course was about dynamical systems; mostly what was then called qualitative theory of systems of first-order differential equations. The second course, built specifically on the first, dealt with (mostly linear) input-output analysis, control theory, and optimal controls. The third was about discrete-time systems, in those days primarily automata theory, regarded as a paraphrase of continuous-time dynamics to discrete situations. The fourth was concerned with spatially extended systems, described by partial differential equations. The fifth, which was in fact never taught because I find the subject uncongenial, was supposed to deal with stochastics. My expositions were built around many examples, as many as possible taken from biological situations, and the emphasis was on making the underlying unifying patterns as conspicuous as I could.

When a student enrolled in the program, I would organize an individual curriculum most consonant with his or her interests. If the student had no interests, I would put him on a reading program of broad scope, until one emerged. Then, and only then, an appropriate curriculum would, so to speak, organize itself around that interest.

I began this program around 1967. At that time, there were almost no coherent text materials I could rely on. So I conceived the idea of turning my course notes into text-books, a digression which I viewed as innocent public service. The notes for the first course were published by Wiley in 1970, under the title "Dynamical System Theory in Biology". The ideas and viewpoints expressed therein have become utterly commonplace today but it was then met with such virulent hostility, especially on the biological side, that I cancelled my plans for the remaining volumes, and vowed never to waste my time on exposition again. What expository work I have done since then has been confined to editing (e.g. a three-volume series, "Foundations of Mathematical Biology" for Academic Press, and the biannual series "Progress in Theoretical Biology", of which seven volumes ultimately appeared.) One of the main thrusts of the latter series was to acquaint English-speaking scientists with the work being done in Eastern Europe and the Far East.

Indeed, the Center itself was always the core of an extensive publication program of its own. The offices of the "Journal of Theoretical Biology" were located there since Danielli had founded it in 1962, and served as its Editor in Chief until his death over a decade later. I remained connected through this time with the "Bulletin on Mathematical Biology", which Rashevsky had founded, and then later, when I got to know Richard Bellman, with his "Mathematical Biosciences". I was also heavily involved with Danielli's authoritative "International Review of Cytology" in those Buffalo years. For various reasons, I have dissociated myself from many of these editorial activities, as these publications, and the policies they now implement, have become less and less congenial to me.

But for a while, Buffalo was paradise. It was at the Center, for instance, that I came to know people like von Bertalanffy, and many others who came through for longer or shorter periods of residence. The Center was destroyed, however, in 1975, in a brutal upwelling of resentments, jealousies, and low parochial politics. But I am sure we all have our academic horror stories to tell. Nevertheless, I continue to regard what happened then as a tragedy for both the field and for innovative university research in general, and it certainly bespoke a catastrophe for SUNYAB, from which it has never recovered.

At the moment, and indeed for at least a decade past, there has been no coherent, broadly based graduate program in this area anywhere in North America. The field seems to prosper, not because of new cohorts of students trained in the area, but by the accretion and immigration of people trained in other areas. Many of these people are technically very adept, but it seems to me that they are producing little in the way of new ideas; what appears in the journals now is primarily the reworking of old ideas, often dating back 30 years or more. Reading them is a dreary exercise, and that is one of the main reasons I have disaffiliated myself, both from the journals which publish them, and from the organizations these journals represent. How can I endorse, for instance, the present editorial policy of the "Journal of Theoretical Biology", when its current co-editor publicly derides the whole endeavor as "trivial", and at best an exercise in combinatorics?

I left Buffalo in 1975, with the closing of the Center, and took up an appointment as "Killam Professor" (so named because it was funded in memory of a wealthy benefactor, Isaac Walton Killam) at Dalhousie University in Halifax, Nova Scotia. This was like a 5 year Sabbatical, which released me for that duration from the strictures of academic politics, and left me free during that time to continue pursuit of my Imperative in good company. I shall always be grateful to Dalhousie for the haven it provided, even though circumstances are much different today from what they were then.

In general, I believe that under presently prevailing circumstances, the best thing I can do, for myself, and for my field, is to pursue my Imperative in my own way, and continue to report. I feel I am [as this was being written, in the early 1990's] much closer to my ultimate goals than I have ever been, and that I can only get stronger as I advance. As I said at the outset, I am not by nature a proselytizer, but my reports are out there, for others to make of it what they will.

If, as I believe, my scientific work comprises a single unity, then that unity reflects the mandates of the underlying unified problem with which I have been concerned. I have tried to listen only to what that problem tells me, and to follow its exigencies. That is the key to how I perceive science itself, and why I have never allowed anyone to tell me how science in general, and Biology in particular, "ought" to be done. Only the problem itself can do that.

If nothing else, I hope to have shown that mathematics and life are not opposites. Most Biologists, I dare say, believe that where mathematics is, there life cannot be, and vice versa. Most (pure) mathematicians, for quite different reasons, feel the same way. But I rather believe that the corpus of mathematics is the ony other thing which shares the organic qualities of life, and provides the only hope for articulating these qualities in a coherent way. But that way is quite different from what has hitherto seemed to suffice for the rocks in this world.

Quite early in my professional life, a colleague said to me in exasperation, "The trouble with you, Rosen, is that you keep trying to answer questions nobody wants to ask." This is doubtless true. But I have no option in this; and in any event, the questions themselves are real, and will not go away by virtue of not being addressed. This attitude, I know, has estranged me from many of my colleagues in the scientific enterprise, and has put me far from today's "main stream". But sooner or later, if I am at all correct, that "stream" will flow my way. In the meantime, I must continue to do what the problem demands of me; as I see it now, it consists of finding a (relational) model, an essence, *all* of whose material realizations must be counted as alive. I think I have indeed found at least one such model; the trick now is to find the objective grounds by which such an assertion can be demonstrated.

As I suspect you have ascertained by now, my relations to General System Theory follow no direct, straight-line trail. There were many sources which fed into it, prepared in many cases by my own independent work, before I had ever heard of General System theory per se.

For instance, I had early been much taken with the "Mechano-Optical Analogy" of William Rowan Hamilton, which seemed to me so different in character from anything else in theoretical physics. Hamilton (whom I consider one of the most original minds of the 19^{th} century; perhaps only Poincaré' and a few others are even comparable to him) did not try to "reduce" optics to mechanics, nor *vice versa*, as Maxwell fruitlessly tried to do later, but rather related them through mathematically homologous action principles. This was an incredibly fertile thing to do; among other things, it led Schrödinger to his Wave Mechanics (which Hamilton himself had all but derived), and, in a completely different direction, to all modern approaches to Optimal Control. I have found many occasions to invoke it myself, in many contexts. Thus, when (much later) I heard

General System Theory characterized as comprising anything bearing directly on independent disciplines, I thought of it as an attempt to do on a broad scale what Hamilton had done in Physics; as a way of relating apparently diverse kinds of systems in a way different from simply trying to reduce them both to a set of common parts. I was also independently familiar with Bertalanffy's development of the "open system" metaphor, which I always viewed as similar in spirit to Hamilton's. That is, diverse systems behaved as they did simply because they were open, not because of irrelevant structural details. In fact, it was largely this metaphor which led me to think of stability as a basic organizing concept, and ultimately to my text on Dynamical Systems mentioned above.

When I came to Chicago, I learned, of course, of Anatol Rapoport's work on random neural networks, done during the decade when Rapoport was a member of the Committee on Mathematical Biology. This too turned out to have application to many diverse subject areas; originally developed to show how specific architectural features could be robustly generated through simple statistical biases (thus freeing these features from the burdens of specifying precise wirings), it gave insight into, e.g., the nature of epidemics, the spread of rumors, and many other things.

So it was that my own independent work and study were leading me precisely in the direction marked out by General System Theory. I became aware of the Society for General System Research, however, only around 1962, when I was asked to have some of my early papers reprinted in one of the SGSR Yearbooks (I think it was the third).

I looked at some of the other papers in these Yearbooks, and must confess I found them disappointing. They tended to start from a premise that "General System Theory" was about something they called a "General System" and spent a great deal of effort trying to characterize what that was. It was not an activity I found particularly germane. Indeed, I regarded the field as General (System Theory), not as (General System) Theory.

In addition, I was by then beginning to travel to meetings and conferences, at many of which people who called themselves "system theorists" were in attendance. By and large, I did not find much common ground with these people, or with what they were doing. That distanced me to some extent from the field itself, though I continued to keep an eye on it.

The situation changed considerably when I met Ross Ashby in 1967, at a 6-week Workshop on Theoretical Biology in Fort Collins, Colorado. On those picturesque surroundings, we had many provocative discussions, and discovered many commonalities of interests and inclinations.

The situation changed still further when, in 1968, Ludwig von Bertalanffy moved to the State University of New York at Buffalo, and was providentially quartered in the Center for Theoretical Biology (thought his appointment was in one of the Social Science departments). I vividly remember meeting him for the first time. When I introduced myself, he impulsively embraced me, like a long-lost brother. A rich and, I think, mutually rewarding symbiotic relation developed between Bertalanffy and myself, and with the CTB at large; it provided a natural home for him, as it had for me. However, he arrived when things were going very sour at SUNY-Buffalo, as noted above, and I have no doubt that the anxieties and uncertainties generated by repudiation of firm agreements by his department played a major role in precipitating the heart attacks which killed him.

I got to understand Bertalanffy's view of System Theory, not only as a scientific, but also as a social instrument. Until then, I had always found the term "General Systems *Movement*" uncomfortable; but that is exactly how von Bertalanffy perceived it. Whereas I viewed the reductionisms and materialisms rampant in biology merely as scientifically inadequate, von Bertalanffy saw them as evil and dehumanizing; in the deepest sense immoral. Animated by his profound love of both science and of humanity, he was inspired to project his view of Systems, governed by ways of relating things rather than stressing differences, into an alternate world view; a paradigm, as he called it, which would offer both science and mankind something better. He viewed his vision, then, not merely as something to be reported, but as a Gospel to be preached.

Von Bertalanffy radiated a simple goodness, a largeness of mind, and a dignity notably absent in those who attacked him so violently, such as molecular biologist Jacques Monod. Monod was typical of the excessively positivist, algorithmic, brute-force people who naturally cluster around the idea that reductionism (or as Monod preferred, "analysis") is all there is to science. Their political counterparts were then called "action intellectuals" who, in practicing their self-styled pragmatism and *realpolitik*, only succeeded in committing blunder upon blunder.

Let me tell one story about Monod, who liked to say of himself "Je cherche 'à comprendre". I met him only once, in 1964, when I attended the International Biophysics Congress in Paris. Monod gave one of the big plenary lectures, and it was about operons. The operon was a functional genetic unit, proposed by Monod and his co-worker Francois Jacob in 1959. These authors had proposed that networks of such operons could account for differential gene expression (i.e. for differentiation) in higher organisms, and had illustrated their thesis with a few simple networks which manifested these behaviors. I was interested in these operons from the outset, for two reasons: one, they were *functional* units, not structural ones; you could not isolate an operon per se and put it in a test tube. Thus, it seemed to me that the molecular biologists were leading themselves into a realm they claimed not to exist; a realm which transcended "analysis". Second, because the operon itself is basically a switch, just like a neuron; an operon network is thus very much like a neural network. But instead of axons or any other material channels for signals, operon networks relied on invisible channels governed by specificities. Moreover, I had shown that the simple "operon networks" proposed by Jacob and Monod to explain differentiation were *identical* with the two-factor nets Rashevsky had published decades earlier, to illustrate how "brain-like" behaviors such as discrimination, learning and memory could arise in networks of neuronlike elements. At any rate, in his talk, Monod stressed precisely these networks, and lamented openly that there was as yet no "theory" of them. This encouraged me to approach him after his talk, to suggest the above to him. He listened with obvious irritation for a minute or two, then cut me off with the statement "I am not an embryologist!", turned on his heel and walked away. I was amazed by this; he really *didn't* want to know. This is why, in my eyes, Monod and his ilk are little, and why Bertalanffy was great.

Experiences like that outlined above with Monod, repeated a hundredfold, convinced me that it is useless to preach to those who *will not* hear, whatever one's Gospel, and equally useless to preach to those who already believe. Besides, my nature is not that of a preacher or advocate. I am, and remain, a practitioner of von Bertalanffy's paradigm, and preach it only implicitly, through that practice. And I practice it because my problem tells me that I must.

There was one exception; I *did* proselytize once. It was around 1974, when George Klir and his dean Walter Lowen, came up to

Buffalo from Binghamton to talk to us at the CTB. At that time, CTB itself was undergoing its terminal demolition, leaving many first-level people without faculty lines. George, by contrast, then had faculty lines without people. So the obvious arrangements were made, despite malevolent attempts by the highest levels of the Buffalo administration to prevent it. Since then, George Klir and I have had many fruitful interactions. In 1981, I believe it was, he persuaded me to assume the presidency of the SGSR for one year, assuring me it was only a ceremonial gesture. However, it turned out there was one small string attached – namely to organize the Annual Meeting. My proselytizing on that occasion was aimed at the system theorists; my message, that they be aware of cognate developments in the sciences. I invited only congenial scientists to speak, and I think it went well. But that has been the extent of my overt attempts at advocacy.

My recent book, "Life, Itself", published by Columbia University Press and released in August 1991, could have been subtitled, "Why I am not a Mechanist." I knew that Francis Crick had published a book with my title about a decade earlier, taking exactly a mechanist stance, but I saw no need to cede the title, nor indeed anything else, to Crick. In fact, I had decided to someday write a book with this title when I was still in my teens, after reading a strange little story by Poe called "The Oval Portrait."

The theme of my book, "Life, Itself", is that Mechanism and Vitalism pose a false dichotomy. Roughly, I argue that the external, public, material world is full of closed causal loops, just as the internal, mathematical world is full of closed inferential ones (impredicativities). The "world" of the mechanism, or machine (or, as I call it, the simple systems), and which I believe is an artificial human limitation on reality, does not allow such loops. Accordingly, as a class, these simple systems are extremely poor, or limited, in entailment and hence extremely nongeneric. I pose this in a number of different languages, each bearing on a different part of System Theory. In particular, I pose it in a causal language, and show that a closed loop of entailment permits a perfectly rigorous notion of *final cause*.

I call a system which is not simple "*complex*". Complex systems cannot be exhausted by any finite number of simple (mechanical) models; they cannot be described as software to a "machine". Life itself is tied up irretrievably with this notion of complexity, which differs from conventional uses of this word, but I could think of no other.

Complex systems constitute, to me, a perfectly objective and rigorous universe, in which there are "enough" entailments for life, anticipation, and many other things to exist. In the simple, mechanistic one, by contrast, they cannot exist; their basis has been eliminated at the outset. Clearly I cannot distill three hundred pages to a few paragraphs; indeed the three hundred pages are highly distilled already. In a sense, the book is as much about System Theory as it is about Biology; the two are so closely entwined that I cannot, and would not, separate them. It is no accident that the initiative for System Theory itself came mostly from Biology; of its founders, only Kenneth Boulding came from another realm, and he told me he was widely accused of "selling out" to biologists. I know that Ludwig von Bertalanffy would be pleased by the effort; I hope he would be pleased with the result of that effort.

EPILOGUE

By Judith Rosen

After my father finished writing this paper, he gave it to me to read, saying, "Here, Jude. Give me your honest opinion, kid. I can't be objective. I don't really feel comfortable working on this kind of thing. It seems almost a conceit writing anything autobiographical." I told him I didn't think so and, after I had read it, said that it may someday help someone a great deal because everyone has to start somewhere; finding dead-ends or having to turn around and change direction aren't failures. But a lot of people just coming up and feeling their way like he did over those early years might mistakenly feel that they aren't capable because of similar obstacles. The fact that my father details some of his own dead ends and detours in this paper makes it clear, for posterity, that it's something that happens – even to someone as focused and determined as he was all his life. Therefore, my strong opinion is that his professional life's experiences detailed in this paper give anyone reading this the reassurance that life is just like that for all of us.

This paper also shows that his strategy, which I think is highly original and extremely effective, was to not only scrutinize each dictum that was offered as a given in any of these disciplines, but look all the way back at what the original creator of the dictum was trying to accomplish and then follow the logic (or "illogic") of the origins of it. He did this with many of the accepted traditions in science. What he discovered by doing so is that a large number of the seemingly ironclad tenets, or rules, of science were merely habits based on flawed premises. That was, in my mind, one of his greatest talents and one of the more unusual aspects to his perspective on the universe.

What his paper doesn't talk about or illustrate is that his life was so much more well-rounded than consisting of just his work. Even though he would say that his "Imperative" was the core of who he was, the truth is that his curiosity and his unusual ability to see the big picture AND the details all at the same time were aspects of him that were applicable to every other aspect of life. His astonishing "sticky-fly-paper-memory" (as he called it) was so much fun to explore; he could retrieve facts from anywhere inside there, in an instant. If you asked what was the gestation of an elephant, he knew. If you asked when was JS Bach born, he knew. If you asked any obscure homework question, he knew, and what's even more amazing is that he not only knew the detail you were looking for but all the background and the context, including dates, places, quotes, connections, and consequences... and he could just pull it out of his memory at will. I have to rummage, at the best of times, saying, "I KNOW the word I need is in here SOMEWHERE!". ...But he could recite pages of a Shakespeare play he had to read for high school and hadn't looked at since. He was a killer at Trivial Pursuit (we had to change the rules for him because otherwise the game would be too short). He was a blast to travel with, because he knew at least a half-dozen languages and was so seasoned a traveller that he was able to handle any and all bizarre travel-related situations, without getting the least bit flustered. He loved to "play". Whether playing involved literal things like piano or organ (Bach fugues were his favorite) or figurative things like hiking up a mountain, going to a fine restaurant, watching an old Pink Panther movie on tv, or a million other things, he was enthusiastic and great company. As far as his ability to be a friend goes, well, speaking from personal experience, I think that was perhaps his greatest talent of all.